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STORAGE

JULY, 1911

TRANSACTIONS

OF THE

ROYAL

SOCIETY OF TROPICAL MEDICINE AND HYGIENE

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TRANSACTIONS

OF THE

SOCIETY OF TROPICAL MEDICINE AND HYGIENE.

JULY, 1911.

VOLUME IV. No. 8.

Proceedings of a Meeting of the Society on Friday, June 16th, 1911.

PROFESSOR RONALD ROSS, C.B., F.R.S., *President*, in the Chair.

ANNUAL MEETING.

The President, Professor RONALD ROSS, took the Chair.

After the reading of the minutes of the last meeting by Dr. CARNEGIE BROWN, which were signed as correct, Dr. HARFORD read the Annual Report and Treasurer's Report, which were passed unanimously. Dr. CARNEGIE BROWN then announced the result of the ballot for the President, Vice-President and Council for the ensuing two years :—

President :

Lieut.-Col. Sir WM. LEISHMAN, F.R.S.

Vice-President :

Sir R. HAVELOCK CHARLES, K.C.V.O.

Council :

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ARTHUR G. BAGSHAWE, Esq., M.B.
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Sir Patrick Manson, K.C.M.G., F.R.S.
Professor E. A. Minchin.
H. B. G. Newham, Esq., M.R.C.S.
Sir Ronald Ross, K.C.B., F.R.S.
L. W. Sambon, Esq., M.D.
F. M. Sandwith, Esq., M.D., F.R.C.P.
Professor W. J. R. Simpson, C.M.G., M.D., F.R.C.P.
H. Stannus Stannus, Esq., M.B., M.R.C.S.
J. W. W. Stephens, Esq., M.D.

Professor Ronald Ross, before vacating the Chair, expressed his thanks to the Officers of the Society who he considered had carried out the business of the Society with great success. He specially referred to the fact that 200 Fellows had been admitted during the past two years, which he thought was a remarkable increase. Professor Ross then introduced Sir William Leishman to the Chair, wishing him every success during his Presidency.

Sir William Leishman on taking the Chair, after thanking Professor Ronald Ross for his kind introduction, said that his first official act must be a proposal to appoint Professor Ross, according to the usual custom, a permanent Vice-President of the Society. The motion on being put to the meeting was carried with acclamation. The Chairman then delivered his address:—

Gentlemen,

I thank you most sincerely for the honour which you have done me in electing me to the post of President of the Society. You have paid

me a great compliment, but you have also laid upon me a great responsibility, which is not made lighter by the fact that, under your first two Presidents, the progress of the Society has been so remarkable. We all realise how much we are indebted to them for the extremely favourable position in which the Society now finds itself, and I count largely upon the prestige which they have conferred on it to assist me in the difficult task of carrying on their work.

Of Sir PATRICK MANSON words of compliment or praise you would all feel to be superfluous; the position which he occupies in tropical medicine is such as I venture to think has seldom been held by any man in any branch of the profession to which the majority of us belong. He is the recognised head of tropical medicine, not only in our empire but throughout the world, and I recall with great pleasure the enthusiastic acclaim with which he was unanimously elected the first President of the International Society of Tropical Medicine by the delegates of all nations at Berlin in 1907. The Society he may, I think, look upon largely as his own creation, and I only trust that his interest in his offspring will remain keen enough to let us enjoy the benefit of his frequent presence at our meetings in the future. I confess to a feeling of some depression when I think of the high standard which he has set in the conduct of our discussions when, from the depths of his wide experience, he was always ready with valuable comment and apposite suggestion on whatever subject was being dealt with.

Of the retiring President, Major ROSS, again, who could say how deep is the debt which the whole of the tropical world owes him, while his successful guidance of the affairs of the Society during the past two years is fresh in all your minds. I only hope that he, too, will continue to find that he has urgent business in London at the time of our monthly meetings, and that he will remember that his presence at our gatherings always gives them additional distinction.

In thinking over the subject which I should choose for my introductory address, I concluded that I might venture to depart from what is a frequent custom on such occasions, the composition of a summary of recent progress in tropical medicine. Such a summary, to be of any benefit, would have occupied your time too long, since, as you are all aware, there is no branch of medicine which has made and is making such rapid strides as ours. Rather, I have thought that I might deal

with the future of the Society itself, and lay before you a few suggestions as to possible modifications or alterations which it might be desirable to introduce with a view to increasing its influence and its benefits to our Fellows. It is very far from my thought to suggest that its youthful career has not been a success or that it has not been wisely and well directed; on the contrary, I think that few if any Societies could show a similar record of steady progress, both in numbers and in the high standard of the contributions and discussions with which we have been favoured. But I feel that it is not enough to *maintain* a high standard of utility but that we should ever seek one still higher, and that no body such as ours can afford to rest satisfied with anything less than constant progress. I may remind you that our position differs widely from that of other medical societies, in that by far the larger portion of our members are resident abroad and can only attend our meetings at rare intervals, if at all. It is true that they receive the printed TRANSACTIONS which faithfully record our spoken words, but I think you will agree with me that this is but a poor substitute for attendance at the meetings. For instance, it must at times, happen to all of us that the subject matter of a particular communication may not be one in which we have any great personal interest, and yet that particular meeting may be more helpful to us than the majority by reason of a conversation which we may have had, before or after the meeting itself, with a fellow-worker. It is this opportunity of meeting one another and discussing matters informally which is so great an advantage, and yet this is one from which the great majority of our Fellows are debarred. I think then that we ought seriously to debate the possibility of introducing some scheme by which the interests of our absent members might be better served than appears possible at present.

We may roughly divide ourselves into those who are connected with or interested in research work and those whose busy lives in the tropics limit their energies to administration or to the prevention and treatment of disease; the latter, naturally, form by far the larger group. We ought then to aim at catering for both sections, and I think that each has it in its power to be of the greatest assistance to the other, if only an organisation was adopted which would bring their respective interests into closer touch. As matters stand at present this intercourse is only possible at our monthly meetings, and I do not advocate

any increase in the number of these, (few of us could spare the time for more), even if we were successful in obtaining sufficient material in the way of papers and contributions.

It must often happen, then, that a member resident abroad in passing through London may find that there is no meeting which he can make it convenient to attend, although he would probably be glad to have the opportunity of meeting fellow-workers from other countries, and might be anxious to obtain the most recent information upon some problem of etiology or sanitation, or the most recent views on the treatment of some disease which gives trouble in his own district. It is true that the information of which he is in search may be gathered from certain journals or periodicals, but you will agree with me that this quest is not easy, even when one is accustomed to such searches through shelves. A simpler way and a better is to go to the man who has made a special study of the subject and to pick his brains of the latest information. But here, again, the diffidence of the seeker, or the preoccupations of the sought, may prevent the communication of the desired information. We all know that nothing could exceed the courtesy and kindness of the staffs of the two tropical schools, and I trust I may add that we are always anxious to help as far as we can in our own College at Millbank, but after all such meetings are at the best infrequent.

Let us turn to another assumed need of our brother Fellow on his transitory visit to London. It may be some years since he was last in England, and he is well aware that things have not stood still in tropical pathology; putting myself in his place I should be anxious to see for myself actual specimens of any new parasites or other material illustrative of recent discoveries which I had only read of or heard of from others. He may not have time or opportunity to go through another course of tropical medicine in one of the schools, and his object would be attained if he had the opportunity of spending a few days, or even hours, over good specimens with a text-book beside him.

Again, there is another need, which I know will appeal to all who are connected with either research or teaching, and that is the lack of material with which to illustrate their teaching or with which to prosecute their researches. It is a commonplace that clinical material in the shape of cases of many tropical diseases is exceptionally hard to come

by in this country. When cases *are* encountered they are, only too often, chronic or convalescent, and useless for the above purposes. The obvious alternative is that the needful material should be obtained from those whom we know to be stationed in districts where the particular disease is endemic, and, thanks to modern methods of collecting and preserving such material, much may be obtained in this way and usefully employed at home.

As a last instance, it may be that, as a result of investigations at home, some line of treatment or of prevention of a tropical disease may appear worthy of being tested in practice, but it is far from easy to have anything of this sort tried on a scale large enough to allow of judgments being formed as to its value, in spite of its publication in some medical journal. What is wanted is the enlistment of the personal sympathy of some tropical medical officers who are practising in districts which afford the opportunity of a satisfactory test being carried out.

These, Gentlemen, are but a few instances which I have brought before you to illustrate how numerous are the points on which we all want help at one time or another, and it seems to me that by utilising the organisation of this young and vigorous Society we might devise means by which our mutual needs could be brought to the notice of those who have it in their power to help us. I am convinced that it is not the will to help that is in defect, but ignorance of whom to help and how to help.

What then can we do? I have certain suggestions to lay before you, but would first ask you to understand that they are no more than suggestions, and it is quite possible that when I come to lay them before the Council, some at least may wither in the calm, cold light of secretarial and other criticism.

The means, then, by which I would suggest that such an extended sphere of utility might be attained by the Society are threefold. First, through an alteration in the character of our TRANSACTIONS; second, by the establishment of a small technical library; and, third, by the formation of type-collections of microscopical and other specimens illustrating tropical diseases, which should be at the service of any Fellow whenever he wished to consult it.

As regards the first of these, an alteration in the present character of our printed TRANSACTIONS, I have found that some of the ideas which

I had formed on this subject have also for some time been in the minds of our valued Secretaries, Dr. CARNEGIE BROWN and Dr. HARFORD, so I have the greater confidence in presenting them to you. In the first place, I think it would be very desirable to open our columns to short articles from Fellows, which need not necessarily have been read at a meeting of the Society. We all know that much valuable observation and information go to waste by reason of the observer being either too busy to commit it to the form of a formal article or paper, or by reason, possibly, of inherent modesty or youth, for I believe that these qualities are neither extinct nor incompatible in the Tropics. Secondly, I would advocate the throwing open of the TRANSACTIONS to correspondence as a means of bringing one's wishes and difficulties to the notice of every other Fellow, with a reasonable hope that one would find in a later number the information of which one was in search. In this connection it should not be difficult, through our Editors, to organise a system by which queries or requests dealing with different departments of our subject should be passed to certain referees who have made those subjects their special study. Through such correspondence columns also an investigator might make his needs for certain material more generally known than is possible even to those of us who have generous friends in many countries.

What I have in my mind is not an attempt to establish yet another Journal of Tropical Medicine but, through the medium of our TRANSACTIONS, to imitate as far as may be that *causerie* which takes place when workers on so many different branches of our subject meet before or after the reading of our papers at the monthly meetings, and in this manner to bring the generality of the Fellows into closer touch with one another, at least on paper, since so many leagues of sea and jungle separate them in the flesh.

The question of the increased expenditure which such alterations imply I shall not touch on, merely hinting that advertisements are a profitable source of income and that we have so far refrained from their acceptance.

Turning now to the second of my suggestions, the formation of a small library, this you will agree is a distinct want in our Society. I purposely call it a small library, for I think it should be strictly limited to publications dealing with the different branches of our subject—

Tropical Medicine and Hygiene. Any attempt to overstep this limit would, I think, be unwise, and also unnecessary, in view of the numerous excellent general medical libraries which are at everyone's disposal in London. In the best of these general libraries, however, it is by no means easy to find all the information of which one is in search, and anyone seeking to study the literature of a particular subject will often have to visit several before he covers the ground satisfactorily. To collect such a technical library cannot of course be a very rapid process, and the means by which it should be started would require very careful preliminary consideration. One of the first things which this library would entail would be a room in which to store it and, as you will see from the Report of the Council for this year, this has already been under the consideration of the Council, since the possession of a room of our own as a meeting place for the Fellows is felt to be a matter of some urgency, and I trust it will not be long before we are able to make some satisfactory arrangement in this direction. We should, I fancy, have little difficulty in arranging to have our TRANSACTIONS placed on the exchange list of similar publications, and I feel that we might count on authors presenting reprints of their papers to the Society once they realised how highly this would be appreciated. Books, also, sent for review, would soon accumulate if it were thought well to include reviewing in the programme of our altered TRANSACTIONS.

The utility of having a room of our own does not, however, end with its advantages as a meeting place and a home for a library, and this brings me to the third of my suggestions, namely, that such a room might also serve to house type-collections of material illustrative of some important branches of tropical medicine and hygiene. In this I have not so much in my mind a museum of the ordinary kind as the organisation of a series of collections of biting flies and insects, microscopical specimens illustrating tropical pathology, helminths, etc., which were intended not merely to be preserved but to be used and handled freely by those who wished to study them; the necessary facilities for the examination of such collections should also be provided.

In making this suggestion I have, naturally, had chiefly in my mind microscopical specimens, since it is with them that my personal work is most closely concerned, so I shall illustrate what I mean in connection with this branch alone, although I feel sure that similar collections such

as those of biting flies and insects, of helminths and their ova, etc., would also appeal to many of our Fellows, and might be of equal or even greater service to the majority.

In the case, then, of microscopical specimens the outfit required is simple enough—a good microscope or two and a few slide cabinets. What I should like to see would be the development of a custom in the Society that all Fellows who come across interesting microscopical specimens in the course of their practice or their researches should stain and mount extra specimens and forward them, with a few words of description, to the Society, where they would be classified, catalogued and added to the collection. This would not really involve them in much labour, since every worker knows how many specimens he throws away which would perhaps be of the greatest value to others, although superfluous for himself. The collection would be fortuitous at first, but, later on, noticeable blanks could be filled in by applying to individual workers, and I have good reason for anticipating that such applications would meet with a most generous response. If this scheme should be taken up warmly I feel sure that it would not be long before we found ourselves in possession of a collection which would be of the greatest value, not only to those who wished to refresh their memories of the appearance of the more usual parasites, but which would also be of incalculable benefit to those engaged in research on many branches of tropical pathology. I need hardly labour this point, but just think for a moment of the case of an observer who has found what he suspects to be a new hæmogregarine or a new trypanosome, if he were able to consult the collection and contrast his parasite with other species, derived possibly from many different sources. The advantages of such a direct comparison over what is more frequent, the study of printed descriptions and more or less accurate plates, need no emphasising. Specimens, also, in illustration of some individual research, such, we will say, as the influence of a certain drug on the morphological characters of a parasite or slides demonstrating the developmental changes which a parasite undergoes in its alternative host or in artificial culture would be of the greatest help to workers on similar lines.

All this calls not only for the generosity of the giver but also for his confidence that his gifts would not be misused, and I would suggest that it be a strict rule that due acknowledgment be made in any work,

founded upon an examination of the specimens in the collection, of the name of the donors, which should always be recorded on the actual specimens as well as in the catalogue or card-index.

Should this scheme bear fruit I see no end to its possibilities of usefulness, not only to individual Fellows but to the progress of tropical medicine itself; but such developments will occur to you all, and I need only add that, personally, I should deem it a privilege to contribute specimens from my own collections of anything likely to be of service to fellow-workers.

You will notice, Gentlemen, that I have only attempted to suggest, in the most general fashion, some of the lines along which it appears to me that the Society might with advantage attempt to progress, having as their main object the provision of increased advantages to the Fellows. It would manifestly oe out of place on this occasion to do more, but I trust I may be successful in carrying you with me in at least a general agreement as to the desirability of some such development. With your support and that of the Council we could at least make a beginning, and I believe that, once started, we should find that once more the old proverb was true, that it is only the first steps which cost. Whatever these first steps may be, and I fancy it would be best to commence with our TRANSACTIONS, we cannot hope to do much or anything without the approval and cordial co-operation of the Fellows themselves, and I feel sure that I may speak for the Council as a whole when I say that we would most gladly welcome suggestions from the Fellows, especially those resident abroad, as to what they feel is most wanted in the directions which I have endeavoured to indicate to you. Such suggestions would be most carefully considered, and would, I am sure, be of the greatest service to the Council or to any Sub-Committee of it which may be appointed to go further into the matter.

Let me conclude by repeating that I feel strongly that although the progress of the Society has been almost unprecedented as regards its membership, it cannot afford to stand still, but must aim at an ever-increasing standard of usefulness. I believe that it can best attain this end by endeavouring to stimulate intercourse and mutual help among its members, especially between those resident at home and those who are stationed abroad. In this way we may make of our Fellowship no vain thing, but the expression of a living and keen desire to help one another,

and in this way to forward the cause we all have so closely at heart, the progress of Tropical Medicine and Hygiene.

Professor Ross, in proposing a vote of thanks to the President for his very practical and useful address, stated that he was highly in favour of the suggestions which he had put forward, provided they were accepted after cool secretarial criticism. He thought that the necessity for a bibliographical department was specially manifest and would be a valuable addition to the work of the Society.

The vote of thanks having been seconded by Dr. LOUDON STRAIN, was carried unanimously.

THE BIENNIAL DINNER.

The Biennial Dinner of the Society of Tropical Medicine and Hygiene was held at the Trocadero Restaurant, under the Presidency of Lieut.-Col. Sir WILLIAM LEISHMAN, the President of the Society, on the evening of June 16th, 1911, following the Annual Meeting. After the usual loyal toasts had been proposed, the prosperity of the Society of Tropical Medicine and Hygiene was proposed by Sir THOMAS BARLOW, President of the Royal College of Physicians, and supported by Mr. BUTLIN, the President of the Royal College of Hygiene, and responded to by the President. The toast of the guests was proposed by Mr. JAMES CANTLIE, and responded to by Sir JAMES PORTER. Professor RONALD ROSS proposed the birth of the President, and with this the proceedings terminated.

Owing to the many engagements in connection with the Coronation season the Dinner was not so largely attended as was expected, but those who were present enjoyed the opportunity of social intercourse, which was much appreciated.

ALASTRIM, AMAAS, OR MILK-POX,*

By Dr. EMILIO RIBAS,

Director of the Public Health Department of the State of Saõ Paulo, Brazil.

Lately there has been prevalent in several localities of this State, on the frontiers of the States of Minas and Parana, a highly contagious and specific eruptive fever, which in appearance resembles small-pox, and while presenting some of the features of this disease, yet it is to be distinguished from it for various reasons.

The disease has been classified by some as chicken-pox, by others as small-pox, while some observers believe it to be a disease not yet included in our nosological tables.

It seems to me that the idea of chicken-pox can at once be put aside, not only because of the immunity conferred by Jennerian vaccination in regard to the disease in question, but also because it shows no preference for children between the ages of two and seven, as is the case with chicken-pox. There is also to be noted a more general confluence of the vesicles and the contained fluid generally becomes purulent.

That vaccination is in a high degree a prophylactic measure, has been proved in repeated instances where the spread of the disease has been checked by this means. On this point I believe all observers are in agreement.

As regards chicken-pox it is well known that vaccination confers no immunity. For the reasons stated the diagnosis of chicken-pox may be excluded, but the immunity conferred by vaccination might lead one to suspect that the disease in question is more closely allied to small-pox.

I am of opinion that it may be the same disease that was observed by Dr. W. G. de Korte, in May, 1904, in South Africa.† There it is commonly known by the name of "amaas," and Dr. Korte designated it as "milk-pox," doubtless owing to the milky appearance of the vesicles at a certain stage in their development.

There is more than one version as to how the disease has been carried and propagated in this country, but I am of opinion that it came by way of Bahia, remembering the direct trade carried on between that

* An abstract of this paper was given by Dr. Loudon Strain, with an exhibit of lantern slides, at the Annual Meeting of the Society.

† *Lancet*, 1904, Vol. I., page 1273. Amaas or Kaffir Milk-Pox.

port and Africa, and the well-ascertained fact that the disease was brought into this State by agricultural labourers from Bahia, who came overland to work on the coffee farms.

Dr. VILLAS BOAS, of Bebedouro, and Dr. LOPES, of Ribeiraõ Preto, who treated the first cases in this State, were struck by the fact that the disease was almost exclusively found amongst recently arrived agricultural labourers on the coffee farms. Still more, these first carriers of the milk-pox showed that they were quite familiar with the disease, and referred to its benign character as well as to its epidemic character; also that the disease was known in the interior of Bahia by the name of "alastrim," meaning something that spreads, and that they had seen numerous cases of the same disease in Bahia.

From Bahia the disease spread to the States of Goyaz, Minas, Matto Grosso, as well as to the Southern States of Brazil.

I now pass on to state the reasons which lead me to consider the disease as one clinically different from true variola.

(1) *The low mortality at all ages amongst those who have never been vaccinated.* In Saõ Paulo the mortality has been approximately 0·5 per cent., and in South Africa it was slightly above 3 per cent. This low mortality is found even where there has been no medical care exercised and where even ordinary comforts have been absent.

Dr. KORTE says: "Out of 97 patients in the local lazaretto, where they were huddled together on the floor without any kind of care, only three deaths occurred, which were clearly caused by the disease."

In the interior of Saõ Paulo it was proved that absence of all care and the bad sanitary conditions of the houses did not in any way aggravate the mortality, and, although these were certainly not models of hygienic requirements, yet every case occurring in them made a complete recovery. In one of these houses there were eight patients at one time, none of them previously vaccinated. One case was of a markedly confluent type. The only food they had was that supplied by the public authorities, and yet all of them made a complete recovery.

(2) *The disease is of less gravity amongst children than adults, and frequently aborts in sucklings.* I saw personally a large number of cases in Bello Monte do Pirajú and the neighbourhood, in company with Dr. CARLOS MEYER. A large proportion were unvaccinated children, and in these cases as a rule the eruption only reached the

vesicle stage. The vesicles were small and discrete, with the appearance of little pearls. The contents were either absorbed or escaped through small openings in the vesicular wall, leaving a slightly macular appearance, which disappeared after the falling off of a thin scab. In young infants at the breast the eruption did not generally go beyond the papular stage, only rarely were vesicles formed. While this was the general rule in children under five years of age, there were cases in which the eruption became frankly pustular. One of these was the case of a recently-born infant, the mother suffering from the disease at the time of birth *(Fig. 1)*. This case is reported by Dr. SAYAÕ as follows:—

Benedicto, fifteen days old, unvaccinated. The baby is found to be peevish, restless and febrile (38° C.). Remained in this condition for two days, when reddish circular spots appeared over the body. These spots became papules, then vesicles, and by the seventh day, pustules. In spite of some vesicles in the pharynx, which manifestly caused the infant some trouble, the child continued to suckle and made a complete recovery in about fifteen days. The mother had the eruption out when the child was born, and the next photograph *(Fig. 2)* shows the baby and mother on the sixth day after birth.

The cases observed in Bello Monte and Avaré in unvaccinated children to which I have referred, show clearly the great benignity of this disease as compared with true small-pox.

(3) *Absence of secondary fever. Earlier formation of pustules. The fetor of small-pox is not observed.* As regards the absence of secondary fever my personal observations agree with those of Dr. KORTE. In all the confluent cases which I saw during the period of suppuration, there was absence of fever.

The mother of the child above referred to was under the care of Dr. SAYAÕ of Avaré, and we observed her for three days, during the pustular stage. Her temperature at the onset and for three days was 39·5° C. On the fourth day of the disease the eruption appeared, and this coincided with a fall in temperature which did not rise again, not even during the pustular stage. The vesicles on the sixth and seventh days became pustular and confluent, but in spite of this and also that she was frankly in the puerperal period her axillary evening temperature did not exceed 37° C. to 37·4° C.

Another clinical feature frequently noted in these cases is the absence

of the fetor which characterises true variola during the period of suppuration. Bacteriological examination of pus removed from vesicles was made in eight cases. In seven the pus was found to be sterile, and in one case were streptococci found to be present.

I examined many cases similar to the one already mentioned as regards absence of secondary fever, but I will only refer to one more.

Joaquin, an unvaccinated half-breed, thirteen years of age, was, of all the cases seen by me, that in which the eruption was most pronounced. Besides the intense eruption over the cutaneous surface, it was also pronounced on the labial, buccal and pharyngeal mucous membrane. In spite of this extensive eruption, on the evening of 13th August, when the vesicles were frankly purulent, his temperature was 37·4° C., or only slightly above normal. This boy made a complete recovery. *(Fig. 3* a somewhat similar case*)*.

Secondary fever does occur now and then, but it is exceptional, and must be considered as an abnormal type, just as occasional aberrations as to temperature occur in yellow and enteric fevers.

Dr. Nilo Cairo, of Paraná, informs me that he has had some cases of a hæmorrhagic type. While admitting that a few such cases have occurred, as well as cases where secondary fever has developed, they are of such rarity as in no way to make me modify the description I have given as to the normal clinical features of the disease.

As a general rule, it can be affirmed that, except in very rare cases of gravity, the patients feel quite well as soon as the eruption appears, the reason for this being that it coincides with the disappearance of the fever, which has been marked during the period of invasion. Frequently, as soon as the eruption appears, patients go on to their usual food and return to their habitual occupations. I met a man on horseback, driving cattle, and observed that he had the vesicular eruption, already frankly purulent. He told me he remained in bed for three days, during which time he had pains in his body and a sore throat, but as soon as the eruption appeared he felt well and resumed his usual work. On examining him, I found the lymphatics in the neck were swollen and painful to pressure, and the buccal mucous membrane was congested.

(4) *The appearance of the resulting cicatrix.* When cicatrices are left by the milk-pox, which is unusual, they present an irregular and shallow depression, the margin also being irregular in outline.

I am quite of Dr. KORTE'S opinion as to the characteristic appearance of the cicatrices.

(5) *The mode of action of Jennerian vaccination.* Vaccination is an excellent prophylactic measure against the spread of milk-pox, although the immunity conferred seems in some cases to be of shorter duration than is the case with regard to small-pox.

One girl attacked by the disease had good cicatrices from vaccination performed two years previously. On a farm in Uberaba sixty cases occurred in unvaccinated persons, and one case in a patient who had been vaccinated during the previous year.

I will now recount my own experiences in the vaccination of those who only recently had recovered from milk-pox.

Vaccination on an extensive scale was carried out at Bello Monte do Pirajú, and both in the village and the farms the result was completely negative amongst those who had had alastrim, whilst in those who were unvaccinated, and had not had alastrim, vaccination gave positive results, with the exception of one case, in which the patient was vaccinated on the second day of the period of invasion of the disease, and in this case the disease was of the confluent type, and at the same time the vaccination vesicles became fully developed.

The same immunity to vaccination was observed in Avaré.

As the earliest cases in these two districts had been seen only from two to five months previously, I sought out others where the attack was of older date. Thus, fifteen patients whose attack dated from six months back were vaccinated, and out of these in seven cases the result was frankly positive, giving a percentage of 46·6. This fact not only is strongly in favour of the differential diagnosis, but is of considerable importance from the point of view of the public health. For if these persons, six months only after recovery, showed such marked susceptibility to vaccination, it is reasonable to suppose that the immunity conferred by the disease is only of short duration. This is a point of importance in dealing with vaccination as a prophylactic measure against the spread of small-pox. Of the seven positive vaccination results, I am able to show a photograph of two showing the vaccine vesicles well developed *(Fig. 4)*.

I ought, however, to state here that rare cases exist in which

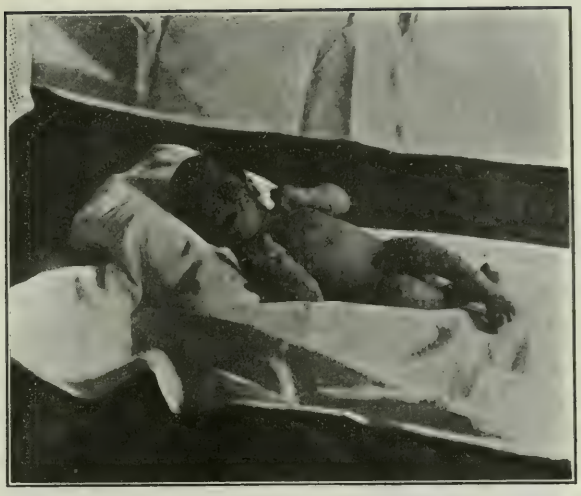

Fig. 1.

Fig. 2.

Photographs illustrating a paper by Dr. Emilio Ribas.

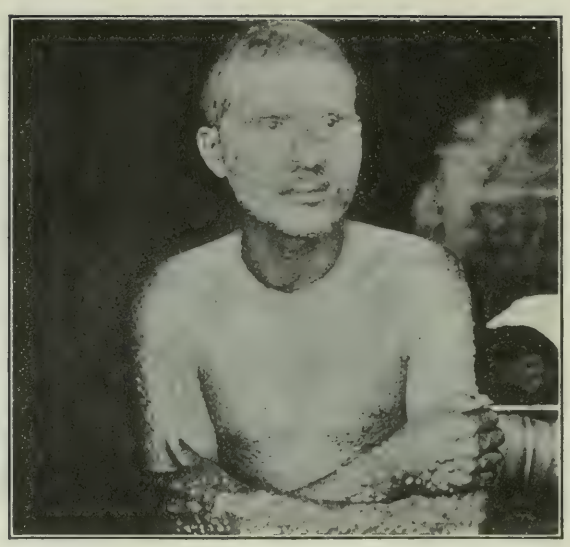

Fig. 3.

Fig. 4.

Photographs illustrating a paper by Dr. Emilio Ribas.

susceptibility to vaccination appears very much earlier. Dr. GOUVÉA, at my request, vaccinated convalescents and recoveries from alastrim, and sent me the following observations :—

C. F., aged seven months. Seen on June 20th with temp. 39° C. The following day a slight eruption was observed on the face, trunk and lower limbs. In eight days the vesicles were pustular, the pustules being slightly flattened in the centre. On July 6th, patient quite well. On August 10th, the child was vaccinated, and two out of three scarifications developed typical vaccine vesicles. At present the scabs are separating. In this case vaccination was successfully performed only thirty-six days after recovery from a typical attack of alastrim.

Dr. KORTE vaccinated fourteen persons—some children and some adults—in the third, fourth and fifth weeks after the date of eruption, and he had one typical result, that of an adult Kaffir. In some only papules formed and in others vesicles. Dr. KORTE sums up his observation thus :—With the exception of the adult Kaffir referred to, in which the vaccination was perfectly typical, all of them reacted to the vaccine in different degrees, like persons more or less immunised by a previous vaccination.

I have now given five characteristics of this disease which help us to make a differential diagnosis between alastrim and true small-pox.

I will now give a clinical description of the disease, pointing out the most frequent symptoms present.

The eruption usually appears on the third day of the illness, first as a rash, which in a few hours becomes papular and vesicular. If the hand is passed over the surface at this stage there is a feeling as if there was small shot in the skin. On the fourth day of the eruption and seventh of the disease the vesicles become " pearly " in appearance, and immediately afterwards they become frankly pustular, and especially on the face. A reddish aureola surrounds the vesicles, but in severe cases this aureola becomes more extensive. The vesicles then lose the pearly appearance and take on a more milky look, or, at times, that of white wax. On the fifth day of eruption and eighth of the disease the vesicles have become pustules. On the sixth day the pustules begin to dry up and form scabs ; some, however, flattening out, do not form scabs.

These scabs are formed in the following manner :—A small point, at first the colour of yellow wax and afterwards becoming darker, occupies the centre of each pustule. This point is the beginning of the change in

texture of the vesicular wall. There a small opening occurs, thus allowing the liquid contents to escape. This drying on the thin vesicular wall forms the scab. I am of opinion that there is no true umbilication as in small-pox; that is to say, in milk-pox this appearance is only present when the above-mentioned change has taken place, which gives a pseudo-umbilication. In small-pox the true umbilication is due to the actual structure of the vesicles.

When the resulting scab falls off, in the form of a small dark-coloured scale, the underlying surface is seen to be slightly *elevated* compared with the surrounding skin. The colour is dark red, but this disappears on pressure, and at the end of a certain time these maculæ disappear altogether. Only in rare instances, and that where the disease has been of a confluent type, do the marks remain permanently.

The eruption also appears on the mucous membrane of the mouth and other parts.

In cases of moderate intensity the duration of the disease may be put at fifteen days, but in severe confluent cases it may last twice this time. Confluence is most frequently observed on the face and lower limbs, rarely on the arms and trunk.

Alastrim has the same incubation period as small-pox, and during the period of invasion there is a rise in temperature till the appearance of the rash. The temperature may reach 40° C. to 41° C. The pulse rate is in proportion to the temperature.

Other symptoms are sore throat, hoarseness, painful swelling of the cervical glands, injected conjunctivæ and coryza. Headache, backache and vomiting are also common symptoms, but the backache and vomiting are not so pronounced as in small-pox. Cases of greater severity occur, and when suppuration is well marked there is subcutaneous œdema, especially of the face. If the eruption appears on the palms of the hands and soles of the feet it is very painful. The urine is normal, and in the cases examined no albumen was ever found.

Taking into consideration all the points I have mentioned, I am forced to the conclusion that alastrim differs from all the eruptive fevers known in this country.

Its true nature and etiology can, however, only be determined by laboratory investigations.

Up to the present all attempts to prove that the disease is a modified form of small-pox have not been convincing.

Dr. Daniels: The subject is an important and difficult one, and deserves the most careful consideration. Previous similar outbreaks have taken place. Dr. Izett Anderson well described an epidemic of the disease in 1864 and called it "Varioloid Varicella."

All through the West Indies three or four years ago there was another outbreak of a disease similar in character to that described in the paper; in fact its appearance led to a good deal of discussion, the Barbadians calling it varicella, and others calling it small-pox. Sir Patrick Manson and others were so inclined to think it small-pox that (whether it was small-pox or not) it was agreed it ought for practical purposes to be diagnosed as such.

The Jamaica epidemic in 1864 was not the first of its kind.

The appearance of the rash in all these outbreaks was typical of small-pox, but as none of the cases were fatal the disease was thought to be a modified varicella and not small-pox. A further reason for this view was that amongst the cases treated by Dr. Anderson there were two patients who were still deeply pitted from an epidemic of genuine small-pox, which had occurred nine years before with a heavy mortality, and if the 1864 outbreak had been small-pox these two cases should have been immune.

There were several peculiar features about the late epidemic which occurred in the West Indies. This outbreak was not so general in character, and race seemed to have something to do with it. Few Indian coolies were attacked, but so many blacks (negroes) contracted the disease that the coolies described it as "black man's disease." This may have been the result of vaccination, as the coolies are all re-vaccinated on leaving India, whilst many of the blacks have not been vaccinated at all, and very few re-vaccinated.

Vaccination often failed to protect in individual cases. One of the hospital attendants who had been successfully vaccinated five months before, has a severe typical attack, but in the majority of cases the attacks were singularly mild after vaccination.

I have shewn Dr. Ribas's paper to several persons who saw the West Indian outbreak, they all state that the cases quoted by him are undoubtedly cases of "Varioloid Varicella."

Partial protection can be obtained by vaccination, but complete protection is very doubtful. In few cases was there any secondary fever.

Before the appearance of the rash the patient may have high fever and be delirious; the rash comes out; temperature falls; and subsequently there is no secondary fever and the patient feels perfectly well, goes about and spreads disease. There are occasional cases amongst these of secondary fever, and only a small proportion of the cases seems to get pitted.

Without giving an authoritative opinion it would be unwise to say definitely that the disease is not small-pox. The vast majority of patients are willing to admit it as small-pox and to go into quarantine.

Opinion amongst those competent to judge seemed to be divided. In Jamaica it was considered as a modified varicella. Some said the disease was a modification of small-pox, but at the end of the last epidemic the general opinion seemed to be that for all practical purposes it ought to be treated as small-pox. Dr. RIBAS appears to consider it as a new disease, and neither small-pox nor varicella.

Dr. G. C. Low: I entirely agree with the remarks made by Dr. DANIELS when he said that the cases described by Dr. RIBAS were similar in type to the small-pox that prevailed in the West Indies in 1902 and the following years. I saw the man who brought the infection to Barbados in the beginning of 1902, and also many of the cases that followed. The particular type of infection was introduced by a white man, who was brought by ship from Canada, where a mild epidemic of the disease prevailed. From this chance introduction the same mild type of the disease rapidly spread in Barbados, then passed from there to Trinidad, where it was for a long time confused with and actually called chicken-pox, and then on to other places—South America (Venezuela) included. The death-rate was extraordinarily low, one or two per cent., and that especially so when it was considered that a large part of the population were unvaccinated. OSLER, in his text book on Medicine, mentioned similar mild cases in the States and Cuba, which, on account of their mildness had also been given specific names, such as Cuban itch, etc. Taking all these points into consideration there is no doubt that Dr. RIBAS was dealing with similar cases, in short with a type of small-pox, and therefore one could not give a new name to his cases. Though this is so there were many points of interest in these cases which would amply repay further work on the subject, and Dr. RIBAS has therefore done service in bringing them again before the medical profession.

YELLOW FEVER IN THE OLD WORLD,

BY

SIR RONALD ROSS, K.C.B., F.R.S.,

Professor of Tropical Medicine, University of Liverpool.

Ever since yellow fever was shown to be transmitted by *Stegomyia fasciata* the question has been before us as to why it is not as prevalent in the Old World as in the New. We have known for years that that mosquito is not only widely spread in the Eastern Hemisphere,* but is often very abundant in certain places. Moreover, the disease, or at least one which is clinically extremely like it, has been reported for centuries as occurring to some extent in West Africa, Spain and elsewhere—even at Swansea in England. We should therefore have thought that, as both the germ and the carrier were present in the Old World, the disease could have spread in it without difficulty.

Side issues of the same question have also been before us. Some years ago the fear was expressed that improved methods of ocean transit, and particularly the cutting of the Panama Canal, would, sooner or later, introduce the disease among the teeming populations of India and China; and quite recently there has been considerable discussion (especially at meetings of the Society of Tropical Medicine) of the suggestion that yellow fever not only occurs in West Africa, but (so far as I understand the proposition) abounds there to such an extent that it is endemic among the native population—rendering them immune and leaving the non-immune Europeans most open to infection. These are evidently different aspects of the general problem. If the disease abounds in West Africa, or even exists there, there is no immediately apparent reason why it should not have spread long ago, say, by way of Morocco and Algeria, into Egypt and thence to East Africa, India and China. From this point of view direct sea-carriage from America would not be necessary; endemicity in West Africa should have led to endemicity in every place where *S. fasciata* exists. Yet there are no grounds for supposing that it does exist, say, in Egypt, India and China. An acute disease with such a high case-mortality would have made its presence felt long ago among the non-immune Europeans in

*See, for instance, Mr. THEOBALD'S paper at the First International Congress of Entomology at Brussels.

those countries. Nothing like yellow fever as it used to prevail in Havana could have escaped attention, for example, in the large cities and garrisons of India. The precise question before us, therefore, is this one—can there be any factor which may suffice to prevent the spread of yellow fever in the Old World, in spite of the presence of *S. fasciata* ?

I was not able to join in previous discussions on the subject because I wished to reserve my opinion until I had completed certain mathematical studies of epidemiology which I was then making; but the Society may now allow me to mention my results briefly without troubling them with the mathematical details. Clearly, epidemiology *must* be studied mathematically if it is to be studied accurately at all. It is not enough to say that an epidemic is due to a complex of factors. We must also try to ascertain how much each factor influences the result. This can be done only by mathematical treatment (which is merely ordinary reasoning rendered as precise as possible); and this is the only way in which we can ever hope to answer the question I have stated.

Yellow fever is one of the large class of metaxenous diseases—that is, diseases due to parasitic organisms which live in two species of hosts. The equations for the whole class are similar, but differ in detail for each disease because of difference in the constants—that is, the pathological phenomena, the immunity factors, the numbers, birth-rate, death-rate, immigration and emigration of both hosts, and the communicative factor (which is, in the case of yellow fever, the frequency of mosquito bites). The equations show the existence of three general laws: (1) The metaxenous disease will not continue to exist in a locality unless both the hosts are numerous enough; (2) A small increase of the numbers of one of these hosts above this point, which may be called the critical point, may cause a severe epidemic among the other species of host; (3) The disease will tend to reach a limit depending on the constants.

Applied to yellow fever the results can be stated very briefly. The mere presence of *S. fasciata*, even possibly in considerable numbers, may not be sufficient to maintain the disease anywhere. The insects must be numerous *enough*—must probably be, on the average, very numerous (the actual numbers required depends on the temperature, length of life, and other factors). To affect a large proportion of the human population, they must be comparatively extremely numerous.

As soon as this fact became clear I endeavoured to obtain evidence as to the comparative numerical prevalence of *S. fasciata* in various parts of the world. Scarcely any estimates were available; and indeed I suspect that even the records of local distribution are as often as not based rather on impressions than on entomological identifications—*S. sugens* and other species being quite possibly often lumped together with *S. fasciata*. In one case the statements made are statistically unreliable. I therefore wrote to Mr. F. V. Theobald, whose experience in the study of large collections from many parts of the world is of course very great, and whose authority on the subject is pre-eminent. He at once informed me that, judging from the huge collections which he has seen since 1899, he should say that *S. fasciata* is far more abundant in America and the West Indies than anywhere else. In India and Asia generally its place, he says, is almost entirely taken by *S. scutellaris*. In Africa it appears to be common only in a few districts; and in Europe it occurs only in small numbers in Italy, Spain, Greece, etc.

This evidence is probably as good as any that we shall ever obtain; and I therefore conclude that the absence of yellow fever in the greater part of the Old World is probably due to the comparative scarcity of the carrier.

It might also be due to a greater natural-immunity factor in Old-World varieties of *S. fasciata;* and I therefore asked Professor Newstead and Mr. Carter of the Liverpool School of Tropical Medicine to see if they could detect any marked difference in specimens from the Eastern and Western Hemispheres. They could find none; and Mr. Theobald tells me that he does not think we can establish any such difference.

Another important point is elucidated by the equations. Hitherto it seems to have been generally held that, because both malaria and yellow fever are mosquito-borne, their epidemiology must be much the same. Now in malaria the parasites live in the human hosts often for long periods, but in the Anophelines, owing to the short life of the latter, only for a week or two (probably). In other words, in a malarious locality we shall find them at any given moment in a large proportion of the human beings, but only in a comparatively small proportion of the Anophelines; so that human beings, especially new comers (including children), form the reservoir of infection in malaria. But on working

out the yellow fever constants according to accepted observations, I was surprised to find that the proportion of infected persons (endemic ratio) must be extremely small in this disease, while the proportion of infected *Stegomyiæ* must be very large—just the opposite of malaria. In other words, even where yellow fever is endemic in a *place* it cannot be correctly said to be always or generally endemic among the *human population*. It must be the *mosquitoes* and not the men which are the principal reservoirs of the yellow fever virus. And this must be apparent after a moment's consideration, without mathematical computation. Yellow fever is an acute disease with rapid case-immunity; and as shown both by man-to-man and mosquito-to-man inoculations, ceases to be infective in about three days after the commencement of the illness. Thus, except perhaps shortly after the commencement of a severe epidemic among *non-immunes*, all the human material is, so to speak, so rapidly used up that few infected persons will be found on any one day, even in a very large population. At the same time, unless the *Stegomyia* supply and the communicative factor are maintained at a very high level, the epidemic may cease with great rapidity. This explains—if the fundamental observations are sound—why yellow fever is principally a disease of large crowded populations with a high *Stegomyia* index, such as Havana was; and we must not for a moment assume that phenomena similar to those found there and in other American cities must necessarily occur also in other parts of the world, even though *S. fasciata* is present, or even abundant.

The phenomenon of a *smouldering* metaxenous disease is due to the presence of one of the hosts in numbers which average just a little above the critical point. These hosts will of course not be uniformly distributed—they will be below the critical point at some spots and above it at others. Thus the disease will occur, so to speak, *in pockets*—will die out completely over large areas and will be just able to maintain a footing in particularly favourable ones, and perhaps to break out in small epidemics during specially favourable seasons. Yellow fever in West Africa comes most probably under this heading, and we must not assume without clear proof, because scattered cases may be detected there, that the whole native population is affected and immunised as in Havana. The case appears to be analogous to that of other diseases, such as that of bilharziasis—which is widely endemic in East Africa, but can only just keep a footing on the west coast of India.

Yellow fever belongs to a group of non-malarial insect-borne diseases —dengue, sandfly-fever, and probably others—the diagnosis of which is often very difficult unless autopsies and microscopical methods are generally used, and unless a marked epidemic is in progress. Hence in "smouldering diseases," where epidemics are slight and rare, diagnosis is so far from easy that numbers of the cases may be returned, even to-day, under almost any heading which happens to be the fashion of the moment—and what must have been done years ago? For example, a case of suspected yellow fever in a ship just returned from Brazil was admitted into hospital under me a few weeks ago in Liverpool. There was high fever which had recently commenced; flushed face, rather slow pulse, and marked albuminuria. No typhoid or paratyphoid reactions followed and no organic mischief was discovered. Yet the temperature took three weeks to fall (by lysis), and a single *Plasmodium vivax* was found on the twenty-second day after admission! Numbers of equally indefinite cases occur in all tropical practice and are classed by the clinician as he pleases. Yet there can be absolutely nothing approaching scientific proof as to their true nature.

I conclude then that the most probable reason why yellow fever has not been able to spread in the Old World is simply that, for some unknown cause, *Stegomyia fasciata* is not so abundant in it as in the New World.

DEATH OF SIR RUBERT BOYCE.

At a meeting of the Council on July 4th, the death was reported of Sir RUBERT BOYCE, which had taken place on Friday, June 16th, the date of the annual meeting, he having been elected as a member of the Council on that same day. The Council passed a resolution expressing their sense of the loss which the Society had sustained in the death of their colleague, Sir RUBERT BOYCE, who had contributed materially to the proceedings of the Society during the past year, and they requested that an expression of their sympathy should be conveyed to the relatives of SIR RUBERT BOYCE, and also to the Liverpool School of Tropical Medicine.

READING ROOM ACCOMMODATION FOR FELLOWS.

For some time past the Council have endeavoured to make arrangements to secure a reading room, where Fellows might meet and consult a Library containing Tropical literature, but hitherto they have been unable to secure such accommodation. The Medical Society of London have now most generously offered to allow Fellows of the Society of Tropical Medicine to use the library and reading room of their Society. This offer, which was communicated to the Council at their last meeting, was cordially accepted, and a vote of thanks passed to the Council of that Society. It is hoped that these arrangements may prove of great convenience to Fellows, and that many will make use of the facilities thereby afforded.